of his own market, his own labor force, and his local sources of raw materials. Most of the accurate data which exist are limited to individual firms, and these are by no means scientifically selected samples. More often than not they are imprecise either because they were collected by inexpert observers or, perhaps, because they are merely the recollections of an aged survivor of his era.

Temin discusses at length (Appendix A) the inadequacies of earlier statistical data, much of which was based on an extrapolation from information filed by relatively few furnaces. Since it is more than likely that the reporting firms often concocted their data (the term is used deliberately) from their records, in some cases adding a figure from motives of pride and in others deducting enough to conceal the true extent of the operations from their competitors, the base for extrapolation is a poor one. Certainly the resultant figures are to be regarded with suspicion.

The formalism of modern economics is well exemplified by a recent pronouncement on the bases of modern economic theory:

> Maximization provides the moving forces of economics. It asserts that any units of the system will move toward an equilibrium position as a consequence of universal efforts to maximize utility or returns. Maximization is a general basic law that applies to the elementary units and, by the rules of composition to larger and more complicated collections of these units. The system is conceived as a mechanism of interdependent parts related by common laws.[2]

The industrial historian is obliged to regard this as an oversimplification and to prefer the warning of George Rogers Taylor: "quantitative studies in economic history, particularly those having to do with growth over appreciable periods of time, necessarily involve, to an extent often forgotten or ignored, basic assumptions and value judgments." [3]

Temin's study should provoke further work in what is still a relatively unexplored area. This reviewer believes that only the study in depth of many individual firms will eventually lead to the solutions which Temin has so valiantly attempted. The historian of innovation in the twentieth century is confronted with the same problem. How, for example, can we explain such phenomena as the slow adoption in this country of such processes as continuous casting and basic oxygen steelmaking? Negative decisions by corporate executives are equally as important as positive ones. How and when can we hope to have access to the data from which to explore these decisions? And when shall we be able to assemble the individual results to discuss effectively the great shifts in industrial techniques in terms of the entrepreneurial decisions which preceded them?

P. W. Bishop

Smithsonian Institution

L. Pearce Williams. *Michael Faraday. A Biography.* xvi + 531 pp., illus., index, notes. London: Chapman and Hall; New York: Basic Books, 1965. $12.50.

The numerous biographies of Faraday have distinctive characteristics: a typical nineteenth-century life-and-letters approach by Bence Jones, an emphasis on scientific achievements by Tyndall, a loose personal account of the man by Gladstone, a social interpretation by Crowther. The need for a new major effort has long been recognized, and this is what L. Pearce Williams now gives us after a mammoth effort of searching out the Faraday manuscript legacy. While the book probably does not achieve all that the author hoped, still it must be considered an important addition to the literature of the history of science.

The best aspect of the book is the excellent description given of Faraday's experimental work; in this respect Wil-

[2] Sherman Roy Krupp, *Functionalism in the Source Science* (Philadelphia: American Academy of Political and Social Science, 1965) (Monograph No. 5), pp. 69–70.

[3] "American Economic Growth before 1840," *Journal of Economic History*, 1964, *26*, No. 4:427.

liams succeeds where Tyndall did not. The account gains immeasurably by the pursuit of a theme which Williams threads through the entire life effort. This theme is basically that Faraday was from the beginning, and continuously, thinking in terms of the space between atoms rather than of the atoms themselves. It was the arrangement of the material particles much more than their isolated properties that determined the interesting characteristics of a body. Williams starts with Davy — in particular with the identification of diamond in 1814 — and emphasizes the continuity of viewpoint in Faraday's work on steel and glass in the 1820's, in ideas about the electrotonic state, contiguous forces, and lines of force in the thirties, all the way through to his later experiments on diamagnetism and magnecrystallic action. No one who has read Williams' earlier works — especially the *Nature* article of 1960 — will be surprised to learn that he equates this concern of Faraday's with Boscovich as the causative agent. And lest one forget, the biographer injects it time and time again: there, see — this is explainable (only) in terms of point atoms. Williams is somewhat embarrassed in this connection by the fact that Faraday nowhere, apparently even in correspondence, mentions the name Boscovich prior to the well-known " Thoughts on ray vibrations " of 1844, and even there it is a very generalized Boscovich system indeed. Faraday announced only that he liked the idea of atoms consisting of point centers with systems of forces extending to infinity in all directions around them.

Since Boscovich was reasonably well known in early nineteenth-century England, and since Faraday was exposed to at least a superficial view of these notions, Williams' theory is attractive. But it is attractive mainly because it provides a starting point where otherwise none can be specifically identified. The idea that space is permeated with forces is so broad, and Faraday's commitment to it so great, that I at least am reluctant to give such complete credit to Boscovich simply because nothing else is conveniently around. In any case it is unlikely that we have heard the last of the Boscovich-influence question.

In describing Faraday as a social being Williams falls considerably short of a definitive study. Neither his marriage nor his Sandamanianism emerges as a real measure of the man. And his relations with other scientists are limited largely to quotations from letters. Several men are called Faraday's friend, including Phillips (a good friend on p. 153), Magrath, Nicholl, Andrews, Snow Harris, Herschel (an old friend on p. 384), Moll, and Schoenbein (to whom Faraday was a dear friend on p. 494), but there is in most cases no indication of how these friendships arose, how strong they were, or what they consisted of beyond the exchange of correspondence.

This book should be on the shelf of any historian seriously interested in nineteenth-century science. It is unlikely ever to be surpassed in its clear account of his work, and while it falls short of producing a comparable understanding of Faraday's character, this may be taken as an indication of the difficulty of such a task. For a real understanding of Faraday's relationships with his colleagues it may be necessary to write a history that embraces more than the biography of any one man.

BERNARD S. FINN
Smithsonian Institution

CONTEMPORARY SCIENCES

Ronald W. Clark. *Tizard.* xvii + 458 pp., illus., list of publications, index. Cambridge, Mass.: M.I.T. Press, 1965. $10.00.

Mr. Clark's life of Sir Henry Tizard will be read for two reasons: firstly, for its account of the organization and disorganization of defense science in Britain from 1920 to 1949; secondly, for its account of individual defense developments with which Tizard dealt, in particular the radar chain.